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Thus it appears that both instruments give nearly the same value for the extent of the variation.

Mr. Henry suggests, as a query, whether this annual fluctuation can be attributed to some annual change in the spirit level. This is not likely: first, because we cannot well see how any change in the zero of this instrument should affect reversed results; and, secondly, because any change of scale, if arising from temperature, could scarcely be so consistent in its indications, and, moreover, could not well affect *mean* temperatures so sensibly while imperceptible at *extreme* temperatures. The cause is not obvious, nor is it quite certain, as yet, that the phenomenon is of universal occurrence.

There is a change of similar nature in the azimuthal errors of both instruments, as will be seen from the tabular statement below. The sign + means, that the telescope points to the east of south, or that the west pier and pivot are too much to the south.

Transit Azimuthal Errors.

	GREENWICH.				CAMBRIDGE.		
Years.	March.	Sept.	Ann^l. Var^n.		March.	Sept.	Ann^l.Var^n.
	″	″	″		″	″	″
1841	+3·22	+2·80	0·42	1836	+11·10	+7·30	3·80
2	4·63	2·53	2·10	1840	0·72	−0·70	1·42
3	5·02	2·55	2·47	1	4·77	+1·33	3·44
4	5·73	3·09	2·64	1842	+3·27	+3·09	0·18
1845	+2·92	+1·53	1·39				
			Mean 1·80				Mean 2·21

It seems pretty certain, from the foregoing tables, that the western Y, in both the Greenwich and Cambridge transits, is about 2″·5 higher at the vernal than at the autumnal equinox, and that it is also about 2″ more to the south at the first than at the second season.

On the Interior Satellites of Uranus. By the Rev. W. R. Dawes.

In the *Monthly Notices* for January last were printed some observations of an interior satellite of *Uranus*, which had been made in the autumn of last year by Mr. Lassell and M. Otto Struve. The results are, in several respects, interesting and remarkable. The fact, that one observer always saw the close satellite on the *northern* side of the planet only, while the other as uniformly observed it only on the southern side, is sufficiently curious to invite further investigation.

It is however obvious, that the observations at Starfield and at Poulkova are utterly incompatible with each other. While the latter point to an approximate period of $3^d\ 22^h\ 10^m$, the period indicated by the former is only about $2^d\ 2^h\ 43^m{\cdot}6$. The *distance*

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also of the satellite carefully estimated by Mr. Lassell on Nov. 6, 1847, under favourable circumstances and with great probability of considerable accuracy, was only 11″; the position-angle being estimated 349°. Now, assuming the direction of the major axis of the projected orbit to be from 10° to 190°, as determined by M. O. Struve for the satellite observed by him; and assuming also, that the apparent ellipticity of the orbit does not greatly differ from that of the orbits of the bright satellites I. and II.; we find that the distance of the satellite, at its greatest elongation, would be 12″·2, on the supposition that the distance was correctly estimated at 11″ when the position-angle was 349°. But this is almost precisely the greatest elongation theoretically due to a satellite revolving about *Uranus* in the period indicated by Mr. Lassell's observations. We are thus led to the conclusion, that there are at least *two* satellites interior to the nearest bright one: and to avoid the confusion which might arise from applying numbers of any kind to the smaller satellites, I beg permission to denominate them for the present, *a*, *b*, *c*, &c., in the order of distance from the primary;—*a* being the satellite observed by Mr. Lassell, and *c* the satellite observed by M. O. Struve.

M. O. Struve suggests that the satellite observed by him may lose much of its light when in the northern portion of its orbit; and this may be the reason why Mr. Lassell did not see it on Nov. 6, 1847; on which favourable night *c* must have been near its greatest northern elongation. On all the other nights when *a* was observed by Mr. Lassell, *c* was very close to the planet, with one exception only, on Sept. 14; at which time *c* was near its greatest elongation southwards, and might perhaps have been seen if the night had been sufficiently good. Neither Mr. Lassell, however, nor myself then observing with him, perceived any such object in that place.

It should here be stated, that the estimated position for Sept. 14, as given in the printed table, appears to be erroneous. It is inconsistent with the diagrams independently made at the time by Mr. Lassell and myself, which, taking the *measured* position of II. as a guide, shew that the position of *a* was about 80° north *preceding*;—whereas the angle as printed is 80° north *following*. It seems clear, therefore, that the angle should be 350° instead of 10°. This being rectified, and the position-angles computed for the times of observation, by reckoning back from the peculiarly valuable observation of Nov. 6 as an epoch, and assuming a period of $2^d\ 2^h\ 43^m{\cdot}6$, the estimated angle *minus* the calculated angle comes out, for Sept. 14 = −2°; for Sept. 27 = −2°; for Sept. 29 = +1°; for Oct. 1 = +2°.

That the satellite *a* should never have been seen by M. O. Struve may arise from its becoming faint in the southern portion of its orbit, as *c* probably does in the *northern*. It appears, from calculation on the assumptions before mentioned, that on every night when M. O. Struve saw *c*, with the only exception of Dec. 10, *a* was in the southern portion of its orbit; yet sufficiently distant

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from his observed position of *c* to render any confusion between them impossible. On Nov. 28, indeed, the position-angles of the two satellites must have been almost precisely the same: but the measured distance of *c* was 16″·85; while the distance of *a* could not have exceeded 11″ in that part of its orbit. It should be noticed, with reference to the non-observation at Poulkova of the satellite *a* on Dec. 10, that though on that night near its greatest northern elongation, it was at a smaller distance from the primary than *c* ever was when it was observed. Unless, therefore, the night were unusually fine, so faint an object might easily be overlooked.

So long ago as the autumn of 1845, Mr. Lassell occasionally saw a faint object, supposed to be a satellite, at about the same distance from the planet as *a* was observed to have been last year; and, with only one exception, it was always seen on the *northern* side of the primary, and usually in the north *preceding* quadrant, in which it was uniformly seen in 1847. On Oct. 5, 1845, at $12^h\ 26^m$ Greenwich mean time, being on a visit at Starfield, I had the gratification of seeing this satellite in the 20-foot reflector. When the light of the planet was hidden from the eye by a bar, the satellite became steadily visible; and a careful diagram being made, both by Mr. Lassell and myself independently, the position-angles deduced from them agreed within 3°, the mean being 324°·1. This is very nearly the same with the estimated position on Sept. 27, 1847; and assuming, that in the interval of 721·8896 days, 342 complete revolutions had been performed, the period comes out $2^d\ 2^h\ 39^m\ 36^s$. It seems probable, therefore, that the period of this satellite does not differ much from that quantity.

Supposing that those nights on which the satellites I. and II. were measured at Starfield, in 1847, were probably favourable, though *a* was not noticed, it becomes interesting to know whether *a* were then at such a distance from the planet as might allow it to be visible in the 20-foot reflector. I have, therefore, computed the places of *a* for each of those nights; and the results shew, that only on Oct. 16 was it in the northern portion of its orbit, and not very close to its primary. On every other occasion, therefore, it was probably either invisible in the southern portion of its orbit, or overpowered by its vicinity to the planet.

By a similar computation, it becomes evident, that on none of those nights was the satellite *c* near its greatest southern elongation; and that, therefore, it was probably invisible on the northern side of the planet, or too near it to be discerned.

It is singular, that in the whole series of observations at Starfield and Poulkova, only one night, Nov. 1, is common to them both; and that was of so indifferent a quality at Starfield as to render it improbable that so difficult an object as the satellite *c* could have been detected: *a* was then in the south following quadrant, and rather near the planet, so that it was not seen at either place of observation.

On the whole, therefore, it seems highly probable, that the interior

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satellites observed by Mr. Lassell and M. O. Struve are entirely distinct; that the one becomes invisible in the southern part of its orbit, and the other in the northern; and that, during the last apparition of *Uranus*, it has so happened, that each satellite has been seen only by its own observer. If either of them is the same as was discovered by Sir W. Herschel, it seems most likely to be that observed at Poulkova.

The object observed at Starfield on Nov. 6, 1847, and supposed to be an intermediate satellite between I. and II., was estimated to be only 10″ distant from the planet. But its position was then almost precisely *opposite* to that of I., whose distance was estimated at 20″. It is, therefore, obvious, that if this were a satellite, its orbit must be *interior* to that of I. It could not, however, have been *c*, which must at that time have been near its greatest *northern* elongation, and therefore probably invisible. Moreover, its greatest distance could be only about 15″¼; and it would, therefore, be nearly intermediate between *a* and *c;* and for the present it may be distinguished as *b*. Though it may seem premature to attempt any conclusions from a single observation of so difficult an object, yet, on the other hand, the observation of Nov. 6 was peculiarly worthy of reliance. The night was unusually fine. The planet was viewed for two hours, during which time the supposed satellites were carried along with it. The distance also of *b* was estimated just half that of I., which was almost exactly opposite to it. And that the estimated distance of I. (20″) was nearly correct, appears from comparing it with the *measured* distance of the same satellite (20″·57) on Oct. 11, when it was in almost precisely the same part of its orbit. If, therefore, it were really a satellite, it appears probable that there are *three* satellites revolving within the orbit of I., at apparent mean distances of about 12″, 15″, and 18″.

Mr. Lassell having favoured me with a communication of all his observations of a close satellite of *Uranus*, with the position-angles estimated at the time, and copies of the diagrams, it appears from them, that only on one occasion has such an attendant been undoubtedly seen on the *southern* side of the planet. This occurred on Sept. 27, 1845, at $12^{h}\ 6^{m}$ Greenwich Mean Time. The estimated angle was 160°, the distance was 3 diameters of the planet from the edge of the disk, which gives 14″ for the central distance. This does not at all agree with any probable period of the satellite *a*. But on calculating the period of *c* from this observation, compared with each set obtained by M. O. Struve in 1847, employing as a guide to the number of entire revolutions the approximate period deduced by him, viz. $3^{d}\ 22^{h}\ 10^{m}$, and giving a weight to the Poulkova observations proportional to the number from which each result is derived, the period comes out from the mean of the whole,— $3^{d}\ 22^{h}\ 8^{m}\ 35^{s}$. Mr Lassell's estimation of distance corresponds to a greatest elongation of about 17″; which, as well as the period, agrees so nearly with the result of the Poulkova observations, as to render it highly probable that the satellite observed by Mr. Lassell on Sept. 27, 1845, is the same as that observed by M. O. Struve.